A numerical model for meltwater channel evolution in glaciers

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This paper provides a novel model of englacial channel formation through the incision of surface streams, and certainly deserves to be published. The mechanism has been proposed previously elsewhere, but has never been tested quantitatively, so the present paper constitutes a significant advance. There are however some corrections and clarifications to the model formulation that should be made first, and the sensitivity to a model assumption that is currently somewhat obscured by the model formulation should be tested. Also, I would question whether viscous creep is the rate-controlling mechanism by which a deeply incised surface channel becomes englacial. I will outline these major points first, and then proceed to list a number of minor issues that should also be addressed.

Firstly, the formulation of the Stokes flow model in equation (1) is incorrect for $n \neq 1$, though I have no doubt that the numerical code actually solves the correct equations. Equation (1) only applies to an incompressible fluid of constant viscosity; a more general form is

$$\nabla \cdot 2\eta \mathbf{D}(\mathbf{u}) - \rho \mathbf{g} = \mathbf{0}$$

where

$$\mathbf{D}(\mathbf{u}) = \frac{1}{2}\left[\nabla \mathbf{u} + (\nabla \mathbf{u})^{\mathrm{T}}\right]$$

Reduction to (1) in the paper follows if η is constant and from $\nabla \cdot \mathbf{u} = 0$, i.e. from

$$\nabla \cdot 2\eta \mathbf{D}(\mathbf{u}) = \eta \nabla \cdot \left[\nabla \mathbf{u} + (\nabla \mathbf{u})^{\mathrm{T}}\right] = \eta \left[\nabla \cdot \nabla \mathbf{u} + \nabla(\nabla \cdot \mathbf{u})\right] = \eta \nabla^2 \mathbf{u}.$$

This does not work if η is not constant, and $\nabla \cdot 2\eta \mathbf{D}(\mathbf{u}) - \rho \mathbf{g} = \mathbf{0}$ together with the definition of $\mathbf{D}(\mathbf{u})$ must be used. Also, it is probably worth pointing out somewhere around here the boundary conditions on the Stokes flow model, presumably of the form

$$2\eta \mathbf{D}(\mathbf{u}) \cdot \mathbf{n} - p\mathbf{n} = \mathbf{0}$$

on the free surface and (?)

$$\mathbf{u} = \mathbf{0}$$

on the remaining parts of the boundary. As for the evolution of the free boundary, see the next paragraph.

Secondly, equation (5) is an equation that has been averaged around the perimeter of the channel, and only makes sense if the shape of A_c is known *a priori* — as would be the case if the submerged portion is always constrained to be semi-circular, as the description of

Nolan and Raymond (2000) suggests (I confess I have not checked that paper). Equation (5) does not make a lot of sense if the channel boundary is to be treated as a free boundary satisfying a local evolution problem, as the description of the numerical method later in the paper indicates. The same is true for equation (7). If my reading of the numerical algorithm later in the paper is correct ('we scale dr_n with the maximal water height (H_{max}) inside the channel (cf. Fig. 2a)' along with the sketch in figure 2a), then the model should really be the following:
A mean melt rate $\bar{m}$ along the channel perimeter is computed as

$$\bar{m} = \frac{\rho_w g(\beta + \gamma)Q}{P}$$

where $\gamma = \ldots$ same as currently in text. *We assume that the local melt velocity at any point along the wetted perimeter depends linearly on depth below the water surface in channel,*

$$m_{loc} = \bar{m}\frac{H_{max} - z}{\int_0^P H_{max} - z \mathrm{d}s}$$

where H_{max} is the instantaneous elevation of the water surface in the channel, and s is arc length measured along the ice surface boundary. The free surface below the water line then evolves as

$$\frac{\mathrm{d}\mathbf{x}}{\mathrm{d}t} = (m_{loc} + \mathbf{u}\cdot\mathbf{n})\mathbf{n},$$

where $\mathbf{n}$ is the outward-pointing unit normal to the surface, and $\mathbf{x}$ is a (non-material) point on the boundary. This last equation also holds above the waterline, if we put $m_{loc} = 0$ there.
I may be wrong in the description above, but it is what the text leads me to believe is coded into the model. In the interest of reproducibility at least this level of detail should be included in the model description.

Now, the particular form of m_{loc} chosen above may have a significant bearing on the rate of channel incision: the way that melting is distributed around the channel should have major consequences for the relationship between widening and deepening the channel. It is not clear that the 'linear' scaling is the 'correct' choice to make. Presumably the actual distribution will depend on the channel shape. For instance, if one were to use the 'shallow channel' model of Ng (Mathematical Modelling of Subglacial Drainage and Erosion, DPhil Thesis, Oxford University, 1998), then m_{loc} would depend not linearly on water depth at a point, but as $m_{loc} \propto (h_{max} - z)^{3/2}$, and one would instead have

$$m_{loc} = \bar{m}\frac{(H_{max} - z)^{3/2}}{\int_0^P (H_{max} - z)^{3/2}\mathrm{d}s}.$$

For a circular channel, by contrast, one would expect that m_loc should be uniform along the channel perimeter (as is suggested by the description of Nolan and Raymond's model in the present paper), so

$$m_{loc} = \bar{m}\frac{(H_{max} - z)^{0}}{\int_0^P (H_{max} - z)^{0}\mathrm{d}s}.$$

Of course, when applied to a partially filled channel, this has the potential numerical disadvantage of m_loc being discontinuous across the water surface (which is presumably a potentially real effect!) and hence undercutting of the channel wall. However, the discussion above indicates that the appropriate prescription of m_loc may not be as simple as suggested in the paper. I am not expecting an in-depth study of this, which is presumably a difficult exercise in turbulent heat transfer. However, a sensitivity study is essential here, and I would suggest using a form of m_loc as

$$m_{loc} = \bar{m} \frac{(H_{max} - z)^\nu}{\int_0^P (H_{max} - z)^\nu \mathrm{d}s}$$

with different values of ν, as the above suggests $\nu = 0$ and $\nu = 1.5$ as plausible parameterizations, alongside the case $\nu = 1$ already considered. I suspect that this will affect the rates of channel incision and hence the depth of the channel at close-off. This is also relevant to the first 'minor point' below — which is actually a major point, if the argument in the paper about further incision becoming impossible when the channel becomes pressurized is in fact correct.

With regard to the close-off process, an anecdotal observation on my own part may be in order. I have seen what appears to be a surface channel becoming englacial at its lower end. To preserve the fig-leaf of anonymity here, I won't disclose the location, but suffice it to say that the closing off process did not appear to be primarily due to viscous closure of the ice above the channel, which presumably requires sufficient depth to drive creep closure. Instead, the narrow and deep channel provided a catchment for snowdrift and sufficient shading to ensure net accumulation of snow during the year, with sintering due to refreezing of meltwater leading to the formation of ice and presumably — I didn't want to risk my life by checking in detail — eventual closure of the channel. It seems to me that this process may well be dominant in practice as it does not require significant depths. A viscous creep process does require significant depth to raise effective pressure around the channel (or simply stress, if you will) to sufficient levels to cause viscous creep. This is probably negligible if the channel is only a few metres deep, at least when compared with potential snow accumulation rates. At least, if the channel is not so wide as to cause snow bridges to collapse before they can freeze. This may be worth pointing out in the text, since there are no actual observations given to support the model.

Some minor points:

- The discussion of the 'analytical maximum depth' on page 2610 had me confused — it was not clear to me initially what would happen if this maximum was exceeded. Would the ice above the channel close off to make an englacial channel? It took me a while to realize (without reading Fountain and Walder) that this maximum depth was the maximum depth at which a closed but unpressurized channel could be maintained. This should be made clear; otherwise this section is a bit of a non-sequitur at this point in the paper. Also why a semi-circular channel? Why not circular? This would make a lot more sense for an englacial channel? Also, in terms of comparison with your numerical output, the assumption of m_{loc} increasing linearly with depth (see above) should be a major factor in this calculation — your model is not really consistent with this maximum depth computation, which (as far as I can tell) assumes a uniform melt rate around the perimeter.

- Colloquialisms like 'Nye's old rat factor' (old??) — page 2610 — should be cut.
- page 2613: '...slope perpendicular towards ...' should probably say 'perpendicularly'.
- page 2613: 'In pressurized channels, radial melting is balanced by radial inward creep of ice, thus no significant downward motion is to be expected (e.g., Röthlisberger, 1972).' It seems to me that here the precise distribution of melting over the channel wall will matter. Also, ice being denser than water, I'd still expect a lowering of the channel to occur.
- page 2615: 'This clearly demonstrates that the temporal evolution of the channel is playing a key role in the simulation as well as the flow regime switch at the end, which both effectively limit the incision behaviour.' What flow regime switch would that be? I was confused by this passage.
- page 2616: '...which indicates that turbulent forced convection is the main heat transfer mechanism.' — I think 'turbulent mixing' rather than 'forced convection' would be more standard fluid dynamics usage.
- Figure 5 — what does the end of each line mark? The point at which the channel becomes pressurized? That would be an essential outcome to underline in the figure caption. Would it be possible to provide a separate figure showing the depth at which channels become pressurized as a function of the parameter β, and possibly α, to compare directly with analytical predictions?